offers, the box is towed by a line at the stern of a vessel which is trading up and down the coast of Chili. It is almost too soon to expect any decisive results at present, but in a few months I hope to be enabled to send both the original copper, and that which has been exposed to the action of the sea.

III. "On the Causes of the great Variation among the different Measures of the Earth's Mean Density." By Captain W. S. Jacob, Director of the Observatory at Madras. Communicated by the Rev. Baden Powell, F.R.S. Received October 25, 1856.

The result of the Pendulum experiments in the Harton Colliery, undertaken by the Astronomer Royal in 1854, and detailed in his paper presented to the Royal Society in January 1856, appears at first sight rather startling, as adding to the already somewhat discrepant measures or estimates of the earth's mean density one more discordant than ever; so that we have now values ranging from 4·7 to nearly 6·6; a range, which, in the absence of any sufficient ground for selecting any *one* as true to the exclusion of the rest, would seem to deprive us of all confidence in their correctness as *measures*, and leave them rather to be classed as *estimates* of a very rough description.

But it will be my endeavour to show, that, while none of the methods employed are capable of giving *strictly* accurate results, the Cavendish experiment is the one which may be relied on as giving a good approximation to the truth, within limits of error (when conducted with proper precaution) far less than those to which either of the other methods are liable.

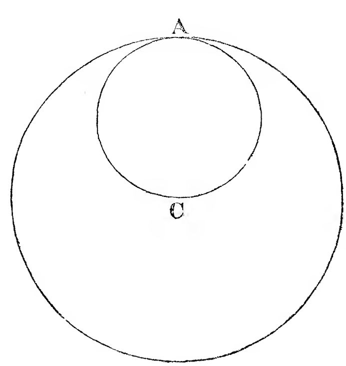
The three principal methods which have been tried are,—1st, the *Schehallien* or *Huttonian*, which consists in comparing the total attraction of the earth with that of a mountain mass, by measuring astronomically the inclination of the normals at a given distance in the meridian-plane on each side of the mass; and then inferring the attraction of the mass from the difference of this inclination from what it would be on an exact spheroid; 2nd, the *Cavendish* experi-

ment, in which the earth's total attraction is compared, by means of the torsion-balance, with that of a small mass of known dense material; and 3rd, the *pendulum*, or *Airy's* experiment, in which the total attraction is compared with that at some distance below the surface, or by means of *differences*, with that of the outer spheroidal shell, whose density may be supposed, approximately at least, to be known.

Now none of these methods give the mean density as a direct result; for the result obtained, the earth's total attraction, is $=g\times$ the sum of (all the particles divided respectively by the squares of their distances) instead of $g\times$ (the total mass divided by the square of the radius or mean distance): and to assume the equality of these, is to assume the earth to be a sphere, and to have its matter arranged in concentric shells or layers of equal density throughout each layer, both of which we know to be untrue. Mr. Airy has indeed shown that, in the case of his experiment, it is sufficient if we know, as regards the upper shell, the form and density of that portion which is in the immediate neighbourhood of the place of observation, without attending to irregularities of distant parts; but he has *not* shown that variations of density *below* and *near* to his lowest station would not sensibly vitiate his results.

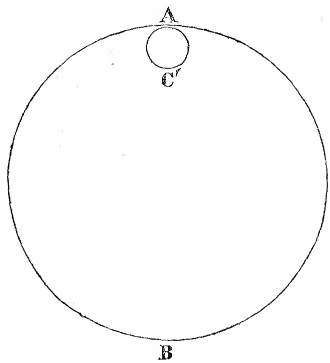
In order to show the nature and amount of error that might thus be introduced, let AB be a section of the earth through the centre, AC an inscribed sphere of half the diameter; then it is evident that on the supposition of a uniform density throughout, the attraction of the small sphere on the point A would be just half of the total attraction of the earth, although its mass would amount to but $\frac{1}{8}$; and if this small sphere were to have its density doubled, the total attraction at A would be increased by one-half, while the *mean* density would be altered by only $\frac{1}{8}$.

Fig. 1.



Now it is quite true that we need not fear any great deviation of density extending through so large a portion of the earth, for that would displace the centre of gravity to a sensible extent, which would become perceptible in the measurements of latitude; but a local deviation might produce a *smaller* but yet *sensible* amount of error; thus if AC′ (fig. 2) represent a sphere of 100 miles diameter, the attraction of this on the point A will $=\frac{1}{80}\frac{dg}{D}$; where D is the earth's mean density, d that of the small sphere, and g the total attraction at A. If, therefore, d should be changed to d', the attraction at A, or the *apparent mean density,* will be altered in the proportion of $1:1+\frac{d'-d}{80d}$; which might be a sensible quantity without producing any sensible effect on the *true* mean density, or on the position of the centre of gravity, since the bulk of AC′ would be only about $\frac{1}{500,000}$ of that of the earth. Now we know little or nothing of the density of the matter a few miles below the surface; only we are sure, from the discordant lengths of the pendulum in different latitudes, and even in the same latitude under different meridians, that the local deviations are indeed *sensible,* yet of so small an amount as hardly to affect this inquiry, and that the error from this cause can never even approach 1 per mille. The Cavendish experiment may therefore be considered as practically free from error of this kind; and as regards errors arising from manipulation or instrumental causes, their probable amount may be determined in the ordinary way from the variation of the results. But if the *Cavendish,* why may not the *Huttonian* experiment equally be considered free from error? Because in the former we are dealing with disturbing masses whose amount is exactly known, whereas in the latter, while we may approximately measure the mass of the mountain *above* the surface, we do not know how much may be added or abstracted

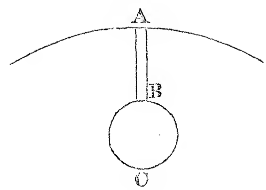
Fig. 2.



below; and we have no right to assume that the mountain is merely a detached mass resting upon the general surface; it will almost certainly have *roots* differing in density from the surrounding country, as has been ably shown by Mr. Airy in the Philosophical Transactions for 1855, page 101.

In the case of the pendulum experiment the uncertainty is somewhat greater; thus, let AB represent the Harton pit, and let BC be a sphere of lead (supposed to lie at its foot) of a diameter equal to AB (*i. e.* about $\frac{1}{4}$ mile); the density of this being about double the mean density of the earth and about quadruple that of the neighbouring country, its excess of attraction on B will be $=\frac{g}{21000}$, while its attraction at A will be only $\frac{1}{9}$ of this, and the difference of its attraction on the two stations will therefore be $\frac{g}{23625}$, being only a little less than the whole quantity observed by Mr. Airy. We may indeed be pretty sure that there is no such mass of *lead*, or mineral of nearly equal density, at the foot of the Harton shaft, yet it is quite conceivable that there should be, within the sphere BC, an excess of density amounting to $\frac{1}{8}$ of that of lead, or about 1·4; and this would produce a difference of effect on A and B amounting to $\frac{g}{144,000}$, and would alter the value of $\frac{D^*}{d}$ to 2·384, and that of E† to 5·96, approaching considerably nearer to Baily's determination. Further, there must doubtless be a small latitude allowed to the assumed density of the upper strata, the average of which, within the limits that would affect the pendulum, may not be exactly the same as in the immediate vicinity of the pit; supposing it to be 2·4 instead of 2·5, E will be reduced to 5·72, being nearly identical with Baily's value.

Fig. 3.



If it should be objected that so large a variation of density as that assumed above (1·4), though *possible*, is not likely; the same effect

* Ratio of mean density of the earth to that at the surface.

† Earth's mean density.

might be produced by a smaller rate of change through a greater space; thus an addition of about 0·5 to the specific gravity of a sphere of 1 mile in diameter, or of 0·33 to one of 10 miles diameter, would have nearly the same effect, and it cannot be contended that these are improbably large.

Should the experiment ever be repeated, it would be desirable to swing the pendulum at one (at least) intermediate station between the top and bottom of the shaft, by which means any error of this kind might be approximately eliminated. In the mean time I think there are hardly sufficient grounds for impugning the correctness of the value of E (5·67) deduced by the late Francis Baily from his carefully conducted repetition of the Cavendish experiment.

IV. "On Practical Methods for rapid Signalling by the Electric Telegraph." By Prof. W. Thomson, F.R.S. Received November 14, 1856.

I am at present engaged in working out various practical applications of the formulæ communicated some time ago in a short article on the "Theory of the Electric Telegraph" (Proceedings, May 17, 1855), and I hope to be able very soon to lay the results in full before the Royal Society. In the mean time, as the project of an Atlantic Telegraph is at this moment exciting much interest, I shall explain shortly a telegraphic system to which, in the course of this investigation, I have been led, as likely to give nearly the same rapidity of utterance by a submarine one-wire cable of ordinary lateral dimensions between Ireland and Newfoundland, as is attained on short air or submarine lines by telegraphic systems in actual use.

Every system of working the electric telegraph must comprehend (1) a plan of operating at one extremity, (2) a plan of observing at the other, and (3) a code of letter-signals. These three parts of the system which I propose will be explained in order,—I. for long submarine lines, and II. for air or short submarine lines.

I. *Proposed telegraphic system for long submarine lines.*

1. *Plan of operating.*—This consists in applying a regulated gal-